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THE PROBLEM OF PHYSICAL PHENOMENA IN CONNECTION WITH PSYCHICAL RESEARCH

BY

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THE field covered by our Society's work is broadly divided into physical and mental, or as Richet preferred to say, objective and subjective. Upon the whole, the Society has turned away from studies of the former kind, not of set purpose, but partly because good cases were difficult to find ; and partly because other lines of investigation yielded more definite results. I think that this policy was and is justified, and if I occupy your attention with a somewhat unfashionable topic, it is only because I happen to be less ignorant about it than about the other and more fruitful branches of the Society's work.

In the time at my disposal I cannot pretend to be exhaustive, and moreover I must confess to being imperfectly acquainted with some of the continental literature, particularly on Eusapia Paladino. For the rest, I believe I have read most of what would be counted of first-rate importance, and will try to sum up the impression I have gained from it.

The weak point of most of the earlier records of physical phenomena seems to be that the observers did not succeed in getting the phenomena sufficiently under control to allow them to have the same effect repeated again and again, so that specific doubts could be

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cleared up by renewed observation. So far as general scientific experience goes, this is the essential condition for getting light on any unexpected effect. Our natural gifts and faculties are not such as to allow us to observe and note everything that happens on a given occasion. Natural limitations make us pick out particular facts which strike us as salient, and we make our reconstruction of the event depend upon them. But, if the selection has to be made in a hurry, and without full warning as to what we are to observe, it is more than likely that we shall make the wrong selection from the observable facts : and then our reconstruction will be altogether wide of the mark : for we shall lack the essential material for a correct reconstruction. The success of the conjurer is one of the best illustrations of this. A good conjurer will almost always refuse to do a trick over again : or, if he pretends to comply with such a request, we may be fairly sure that he will really use quite a different method of producing the same effect : for he knows that if he really repeats his trick, he gives the observer an enormously improved chance of picking out the essential facts. What passed muster as a chance occurrence the first time will not do so again. Attention is concentrated on the apparently irrelevant circumstance, and this sets the observer thinking in the right direction. He arrives at a provisional hypothesis, and this may suggest to him a further question, to answer which he requires to see the performance a third time, or even more.

I believe all this will be much clearer if it is illustrated by a concrete example. As I have said professional conjurers will not repeat their tricks on request : but the regular performer on a public platform is at a disadvantage in this matter, for his repertoire is limited, and he must repeat his tricks : so that the earnest seeker after truth can see them as often as he pleases. For two generations Messrs. Maskelyne had a regular afternoon exhibition of first-class conjuring : and a young man of my acquaintance made it a hobby to try to penetrate their secrets, not always without success. In one of their tricks a man was placed on a kind of chair, with his knees doubled up to his chin. What purported to happen was as follows : A canvas cover was placed over him, but the audience could see underneath ; and, moreover, an observer from the audience was behind. The right hand protruded through a hole in the canvas, and was touched by a member of the audience. The left arm was held up by a string attached to a strap buckled round his wrist, and this string passed out over pulleys to another member of the audience, who held it taut. At the word go, the right hand was withdrawn, the string attached to the left wrist became slack, and on removal of the canvas cover the man had disappeared. Where had he gone to?

My young acquaintance was completely mystified the first time he saw it. The second time he noticed that during the preliminaries, the canvas covering, which was extensible concertina fashion, and was lowered from above, was allowed to fall, as if by accident, to the floor for a moment, being then pulled up again. He vaguely remembered that the same thing had happened before, but since the right hand protruded from the opening throughout, he had not doubted that the man was still there, and had attached no importance to the incident. But the second time set his mind working, and he was inclined to regard this as a salient fact. He went home and thought about it. Was there no possibility that the man had escaped through the floor while the canvas was down? Perhaps the hand was not really his hand, though it was not easy to see how it could be otherwise. Anyway this point could be tested. Next time he went up on to the stage as an observer from the audience: when invited to examine the man who was to vanish, to see that he was not a "collapsible automaton", my friend took occasion to examine closely the characteristics of the finger nails of the right hand, whether they were long or short, clean or dirty, and so on. When the hand was thrust out for him to hold, he saw at once that it was not the same. He noticed further that the front legs of the chair were of hollow metal work—heraldic animals or something of the sort, so that an arm might readily be thrust through from below the stage, and allow the hand belonging to it to come out through the slit in the canvas screen. Thus the mystery was practically explained, for I need not pause over the details about the other hand. The moral I wish to emphasise is that the essential facts that the canvas screen momentarily descended to the ground, and that the front leg of the chair was big enough to contain a human arm, were displayed quite openly to the observer. But he did not at first see that they were at all relevant, and they did not fully penetrate his consciousness: so that he could not use them in his reconstruction of the scene, and he reconstructed it in a way that in fact depicted an impossibility.

The great, and indeed almost the only possible security against this kind of thing is that the phenomenon under investigation should be seen again and again, with full consideration between whiles, in order that any doubtful points in the observer's conception of what occurred should be noted, and the omission repaired on the next occasion.¹ I have, for the sake of clearness, used as an illustration a case where deception was admittedly being practised. But the

¹ A cinematograph film of the whole series of events would no doubt be equivalent.

same applies in all cases when we wish to give an accurate account of observed matters of fact, even when no question of intentional deception enters—*e.g.* in the ordinary researches of the physical laboratory. No experienced worker is satisfied by doing an experiment once; or even twice. If he sleeps on it, he will usually find that doubts have arisen by the morning, and he will perhaps repeat the observation many times, improving the conditions if possible, so as to concentrate on any weak points which have occurred to him.

After a somewhat prolonged study of the early accounts of observations of physical phenomena, particularly those observed with D. D. Home, the chief criticism I have to make is that they are deficient in this regard. Mr Podmore¹ usually emphasises that the description is inadequate for the reader to judge of what really happened: and he interprets the omission in the way least favourable to the phenomenon: indeed, to my mind, he strains the available data in this direction, suggesting for example that if a phenomenon is not stated to have occurred in the light, it must be assumed to have always occurred in the dark. We could easily make mince-meat of many of the classics of scientific investigation, if we allowed ourselves to criticise them in this sort of way. Upon the whole, the observer himself must be the judge of whether he had enough light to see what he describes, though no doubt an explicit statement about the light is desirable. I do not think that an attempt to reconstruct the inwardness of what occurred from the written account is likely to be often fruitful, even if it has been so occasionally. It is impracticable to record *every* condition of a phenomenon in case it might be wanted. Would anyone draw the moral from the case I have given above that we must always describe the legs of the chairs and tables used, or examine finger nails in case these details might be relevant? Clearly not. To do this kind of thing would over-burden the account intolerably. The essential thing is that the observer himself should detect when his account requires to be supplemented, and himself supply the omission: and he can only do this if he can get the opportunity to repeat his observation, if possible under improved conditions.

Upon the whole, the well-known accounts of observations made in the presence of D. D. Home do not show the observers as alive to the importance of this; and so far as they are deficient in this respect, they are to my mind subject to a heavy discount. Lord Dunraven indeed tells us that he was brought into contact with

¹ *Modern Spiritualism*, London, 1902; and *The Newer Spiritualism*, London, 1910.

Home by accidental circumstances and that he did not feel himself specially qualified or inclined to pursue the subject in its scientific aspect: when the first sensation had worn off, his interest seems to have diminished.¹ The Master of Lindsay (afterwards 26th Earl of Crawford), one of the other chief observers of D. D. Home's phenomena,² did not give very systematic accounts of what he had seen. He gives indeed general assurances that he had been able to satisfy any doubts he may have originally felt, but he does not seem ever to have contemplated a detailed dissection of *e.g.* the phenomenon of levitation, such as was afterwards made by Dr Crawford of Belfast.³

When we come to the investigations of Sir William Crookes,⁴ we find, as might be expected, more attempt at systematic investigation. Experiments were planned to improve the conditions, and clear up doubts and obscurities. For example, in reference to "materialisations" Crookes made repeated attempts to see the medium and the materialised figure together, and considered that he had succeeded. This, however, was effectively criticised by Podmore.⁵ Further, he took photographs of the "materialised" figure. The unexplained movements of a pivoted lever obtained in the presence of D. D. Home were recorded graphically on a smoked glass, and various experimental precautions were taken to improve the conditions under which the other phenomena were obtained.

Whatever may be thought of its conclusiveness, there is no doubt that this work represents a definite advance in the spirit in which it was conducted. It does not go far enough however. As well as can be judged, much of the time was spent in waiting for fresh marvels,

¹ *Proceedings, S.P.R.*, vol. xxxv, pp. 21-25, 1926.

² *Report on Spiritualism of the Committee of the London Dialectical Society*, London, 1873, pp. 206, 213.

³ I have asked the present Lord Crawford whether any unpublished letters or memoranda of his father's on this matter survive. He kindly made search but failed to find any. He has also written to me of conversations with his father, who adhered to the position that he had seen levitation in Home's presence, but coupled this with the impression that Home was a charlatan. It is difficult to know what to make of this, but I think it must tend to discount the force of his evidence in favour of the phenomena.

It need scarcely be said that the work of the late Dr W. J. Crawford of Belfast is not connected in any way with the experiences of the late Lord Crawford with D. D. Home. The identity of names is merely a coincidence.

⁴ *Researches on the Phenomena of Spiritualism*, London, 1874.

⁵ On the ground that this seance was not in Crookes' house but at the medium's own home and that the medium might have been personated by a confederate. See *Modern Spiritualism*, vol. ii, p. 155.

under easy-going conditions, rather than in trying definitely to confirm and elucidate those already obtained. I am far from saying that Crookes was to blame for this. It is quite likely that he pressed the policy of persistent investigation as far as he was allowed. The same may be said of many subsequent investigators, who would no doubt have liked to push their enquiries in the direction here advocated, but found that they were not given a chance of doing so.

This brings us to near the close of the epoch of D. D. Home and his contemporaries. Many striking facts had been recorded, by apparently trustworthy witnesses. Their evidence has been discussed at great length by numerous critics, and the utmost has been done to shake it: but, upon the whole, with only limited success. The statements then made still constitute a *prima facie* case for investigation, and it is hard to see how anything that can be done in the future, however negative, can altogether destroy the effect of these records: for failure to repeat can hardly constitute a disproof, when the identical conditions cannot be re-established.

Up to this point some of my hearers may think that I have been over lenient, if not credulous, in my estimate of the evidence. Perhaps what I am now about to say may be a partial corrective. Even if it should eventually become possible to establish the reality of phenomena of this kind to everyone's satisfaction, it would be of very little use, unless we could succeed in going further, and fit them into their proper place in the general scheme of things. Merely to realise that there are more things in heaven and earth than are dreamed of in one's philosophy is hardly an end in itself. The end should be to expand one's philosophy so as to include them.

Consider the phenomenon of lightning as an illustration. There has of course never been any doubt about the reality of this, though in early times it appeared so anomalous and unaccountable that it was regarded as a direct sign of the anger of the gods. (Then, as now, there was a tendency to jump to the conclusion that unexplained phenomena were to be attributed to the arbitrary action of invisible beings.) The observed fact did not at all contribute to the general edifice of scientific knowledge. It lay about like a loose stone of odd size, and served no purpose, until Franklin, by his experiment with a kite, demonstrated that lightning was an electrical phenomenon, and thus fitted the loose stone into the general structure. Builders take no interest in stones of odd size until they can find a place to fit them into. That gives the key to why our studies are so little regarded by the scientific world.

We may go a step further, and consider the more obscure phenomenon of globe lightning, which, as described, consists of detached balls of fire. My father made reference to this in his presidential address to the S.P.R. in 1919. Since he wrote, many more cases have been recorded, and photographs have been published¹ which, while not altogether in accordance with some of the descriptions, seem to prove the objective reality of occurrences having this general character. But they are scarcely understood any better than before. The reason of failure is no doubt that we cannot command them at pleasure so as to devise and carry out experimental tests. That is the same kind of difficulty as we meet with in connexion with our own problem. Some people think it is in itself a ground for incredulity: but this is a train of thought which I have never been able to follow. What they probably have at the back of their minds is that when an effect occurs sporadically, at a time that cannot be foreseen, it is not likely that the best witnesses will be on the spot, and even if they are, they may be taken by surprise, and cannot confirm or revise their impressions of what happened by frequent repetition, as I hope I have convinced you it is essential to do.

In the early days of the Society much attention was paid to physical phenomena, and now the spirit of persistent investigation became dominant, largely under the influence of Mrs Sidgwick, and, later, of Dr Hodgson. The investigation turned chiefly on the question of whether control was adequate or not, and did not get much further. It must be admitted that the results were disappointing, for after spending much time and trouble, especially in connexion with the medium Eusapia Paladino, no definite result was arrived at which commanded the general assent of prominent members. It was indeed common ground that Eusapia had been caught fraudulently evading control, and almost common ground that she must have assiduously practised the methods of deception which she used; but here agreement ended. Some, including Sir Oliver Lodge, Mr Everard Feilding, Mr Hereward Carrington and Mr W. W. Baggally, were convinced that on occasion she exhibited genuine phenomena of telekinesis and the like. On the other hand Dr Hodgson, Mrs Sidgwick and Mr Podmore remained unconvinced.

Since those who were concerned with the actual observations were not able to agree, it is not likely that any subsequent armchair study will help to clear the matter up. My own feeling is that the effects which were relied on by the believers did not differ enough from those generally admitted to be fraudulent to afford any firm basis on which to build further.

¹ Jensen, *Physics*, 4, p. 372 (1933).

We come next to the investigations of the late Dr W. J. Crawford of Belfast with Miss Kathleen Goligher as medium.¹ Dr Crawford was a lecturer in mechanical engineering at the Municipal Technical Institute of Belfast, and from enquiries I have made he seems to have impressed other scientific men favourably. The medium usually sat with a circle of her own friends and relations, a fact to which due prominence must be given.

Dr Crawford's work was largely directed to determining the mechanical reactions of the forces which came into play in the levitations he observed. For this purpose he placed the medium on a weighing machine, to determine whether the seat of the reaction was on her. He worked by the light of a red lamp, of which more will be said later.

Dr Crawford describes how he was able to have a table weighing about 10 lbs. levitated and kept steady about eight inches up in the air for as long as he required to make a test of the addition to the ordinary weight of the medium. This was two or three minutes, and apparently he could have had more, for on each occasion he indicated that he had finished. It was found that the medium gained weight about equal to that of the table. These steady conditions could only be obtained after the sitting had continued for some time.

Dr Crawford interprets this gain of weight by the medium as due to an invisible cantilever or rigid bracket, which comes out from the body of the medium, and supports the table. Mrs Sidgwick, in a review of Dr Crawford's first book, hints that this "cantilever" is nothing else than the medium's leg. Many statements in Dr Crawford's various publications, however, are definitely at variance with this hypothesis. Thus (*E.P.S.*, p. 119) "Practically no palpability is experienced when one cuts through the psychic structure with the hand, or, say, with a piece of wood."²

Dr Crawford's hypothesis is, however, so fraught with mechanical difficulties that it is questionable whether it really helps much to correlate the facts he has determined, assuming that these latter are correct. To begin with, it is almost self-contradictory to postulate a structure which is rigid to act as a cantilever, and not rigid at all for the hand or a piece of wood to pass through it. The attempt to imagine a medium rigid for some purposes but not for

¹ Recorded in his three books: *The Reality of Psychic Phenomena*, London, 1919, here referred to as *R.P.P.*; *Experiments in Psychical Science*, London, 1919, referred to as *E.P.S.*; *The Psychic Structures at the Goligher Circle*, London, 1921.

² See also *R.P.P.*, p. 87.

others is not new to science. Problems of this character arose in connexion with the elastic solid theory of the luminiferous ether, which was to show rigidity for carrying rapid transverse vibrations, and fluidity to allow solid bodies, *e.g.* the earth in its orbit, to pass through.¹ Lord Kelvin at one time appealed to the properties of bodies like hard pitch or cobbler's wax, which while reacting to very rapidly alternating forces, will yield viscously in time to forces applied steadily in one direction.² But to satisfy Crawford's hypothesis the requirement is that there should be great stiffness for persistent forces, and fluidity for more transient ones. This is too much to ask.

Another difficulty is boldly stated by Dr Crawford (*E.P.S.*, p. 117). "How can it be", he says, "that a rigid structure two or three feet long can issue from the medium's body and support 30 or 40 lbs. weight at its end, and the medium experience no inconvenience?"³ Dr Crawford has his own tentative answer to this question, though I cannot personally feel satisfied by it. But in this and other instances the candid way in which specific questions are faced produces a favourable impression, compared with the mere appeal to mysticism of so many writers on these subjects. Dr Crawford's theory perhaps raises more difficulties than it answers; nevertheless, if work of this kind is ever satisfactorily built into the scientific edifice, I do not doubt that he will rank as a pioneer.

¹ A short explanation of this matter may be useful. When it had been established that light, like sound, was of the nature of a wave movement, it was considered necessary to postulate a medium in order to convey it. If the waves were waves of compression, as is the case of sound travelling in air or water, then a fluid medium would do, and there would be no particular difficulty in understanding how solid bodies could pass through it. But waves of compression could not account for the phenomenon called the polarisation of light. When a ray of light passes through a suitable polariser, such as a tourmaline crystal, it acquires "sides" as Sir Isaac Newton expressed it. It is no longer an indifferent matter if the beam is rotated on its own axis. We can prove this by a second tourmaline crystal. This will only transmit the beam if it is placed parallel to the first tourmaline. If crossed with the first there is no transmission.

Now waves can only give room for effects of this kind if they are transverse to the direction of propagation. It is clear that there can be nothing of the sort in compressional waves for in these no one transverse direction can have preference over any other. It was therefore concluded that the vibrations were transverse. But (apart from what happens at a free surface) fluids cannot transmit such waves. Elastic solids can do so in virtue of their stiffness. Hence the elastic solid ether.

² I need scarcely say that the elastic solid ether is now superseded by quite a different order of ideas.

³ This of course applied to a different experiment from that already referred to with the 10 lb. table.

Space is lacking to go into further particulars of Dr Crawford's work. It is necessary to mention that he died by his own hand before the whole of it was published. I shall return to this point a little later.

About a year after Dr Crawford's death, the late Dr E. E. Fournier D'Albe proceeded to Belfast and had a series of sittings with Miss Goligher, with a view to confirming and extending the work of Crawford,¹ which had impressed him favourably. He failed to obtain any phenomena which he could regard as evidential. It was apparently admitted that they were not so (p. 43). Although he expressly reserves the question of whether *any* of Dr Crawford's results could be accepted as supernormal it is pretty evident that he thought they could not be. Towards the end Crawford had obtained numerous photographs of what he regarded as "psychic structures" rendered visible under special conditions. They are published in his last book. The half tone blocks made from these photographs are undoubtedly very suggestive of pieces of muslin or the like, hung from the bottom of the table or knotted on to its legs. Fournier gives similar pictures of much better definition published as actual photographic prints, not half tone blocks, and I fully agree with him that they show the material to be a woven product. This is the most damaging feature in the whole case.

Fournier also tells in detail how he saw the medium raising a stool with her extended foot. Fournier worked as far as possible under the same conditions as Crawford. He had the same circle of sitters, and in one instance actually held the sitting in Dr Crawford's house. He also used the identical appliances, lent by Mrs Crawford. These circumstances are of some importance because they show that she remained on friendly terms with the medium, and can scarcely have attributed Dr Crawford's collapse to his having been ultimately convinced that the medium had deceived him. Dr Crawford stated in his posthumous letter that this was not the reason of his breakdown, and I, for one, accept his statement.

No really valid reason seems to be known for doubting the candour and accuracy of either Fournier or Crawford, so far as they are describing what they themselves observed. Fournier says (pp. 48, 49): "I have no reason to doubt the conscientious and accurate character of Dr Crawford's observations and records."

Fournier thinks that Crawford was too soon convinced that all was well, and relaxed his vigilance prematurely. But Crawford's letters written during the last few months of his life, and given by

¹ *The Goligher Circle*, London, 1922.

Fournier (pp. 66-70), negative this view, for they are full of details of the various precautions which he took.

Fournier emphasises strongly that the shadow of the table afforded protection for fraudulent manipulation, but there are passages in Crawford's books which seem to be a complete answer on the point. Thus (*R.P.P.*, p. 13): "Even with the largest table it is sometimes possible to see completely underneath (as I have done), to see the feet and bodies of all present at rest, and hands held together in chain order, while the table has been steadily levitated."

Again (*Psychic Structures*, p. 8): "A strong red light was falling upon the space below the levitated table while another source of red light was showing from behind so that the whole area between the medium and the levitated table was itself quite visible, and I shifted my position into various positions in the circle, looking at the space below the table from different angles. But to all appearance the space was empty. . . ."

If we accept this statement as being, in Fournier's own words, "conscientious and accurate," I think it is clear that his criticism fails.

Fournier says (p. 49): "The tests to which he [Crawford] submitted the medium completely satisfied him as to her *bona fides* so that he no longer thought it necessary to control the other sitters as well." It is, however, instructive to compare this with a passage from Crawford (*R.P.P.*, p. 16): "The experiments in Chapter III show conclusively that while the table is steadily levitated nearly the whole of its weight is upon the medium. Therefore it follows that if anyone is lifting the table with any part of his body, it is the medium, and the others are not concerned."

Moreover Crawford states (*R.P.P.*, p. 81) that he was allowed to move anywhere between the sitters and the levitated table *except immediately in front of the medium*. If this statement too is accepted as "conscientious and accurate", it is difficult to see the force of Fournier's remark above quoted, in which he suggests fraud of the sitters as the factor neglected by Crawford: the more so that he claims to have seen "levitation" achieved by the medium's foot. Fournier does not seem to have really made up his mind whether the table was fraudulently raised by the medium, or fraudulently raised by the sitters.

I must frankly admit that I am unable to sum up this case to my satisfaction. Fournier does not profess to discuss Crawford's work in detail, and he seems tacitly to admit that for all he can say some part of it may have been correct, though he evidently does not think so. Crawford's publications contain a complete answer to

Fournier's general objections, and I am unfavourably impressed by Fournier's failure to notice this. On the other hand, Fournier does seem to have proved that the medium was on occasion fraudulent. It is difficult to understand what could have been her motive in continuing to deceive Crawford for the first three years, during which there was no payment. It is also difficult to discount either witness. Crawford is confirmed on the main points by several other observers.¹ Fournier stands alone, but produced his photographs, showing the woven texture of what purports to be a "psychic structure". Both records are very satisfactory in point of detail and internal consistency, standing far above the available accounts of D. D. Home in this respect; and the very matter-of-fact style of Crawford's narrative makes any idea of hallucination seem altogether out of place. In this unsatisfactory position I must leave the case.

Finally, I come to Rudi Schneider. A great deal has been written on this medium, much of it of a polemical character. It would be tedious and unprofitable to go over this in detail, and discussion must be limited to the work of MM. Eugene and Marcel Osty in Paris,² passing over other investigations with the remark that I am not convinced by the evidences of fraud which have been put forward. I shall, however, give due consideration to the question of whether fraud could have affected MM. Osty's results.

MM. Osty initially fixed their attention on the phenomenon of telekinesis, or moving of objects by an unknown agency in the presence of the (controlled) medium. It is generally known that a beam of light can be used to guard a treasure, a warning bell being rung when a potential thief approaches too near and intercepts the beam. The method depends on an obvious application of the photoelectric cell, a device which passes an electric current when light falls upon it.³

Now it occurred to MM. Osty that by using a beam of infra-red light instead of visual light the method could be carried out in a dark seance room and that the object (flower, handkerchief and the like), proposed for telekinesis could be guarded by the beam, so that if the medium succeeded in escaping from the controller who was by

¹ See in particular Whately Smith, *Proceedings S.P.R.*, vol. xxx, p. 314, 1920.

² *Les Pouvoirs Inconnus de L'Esprit sur la Matière*, Paris, 1932.

³ It is necessary of course to arrange a relay to switch the bell current on when the photoelectric current is cut off.

way of holding him, and attempted to seize the handkerchief in order to move it, a bell would ring and give warning.

It was found in fact that the bell did ring on frequent occasions when the medium was, or purported to be, in trance. The ringing was sometimes maintained for half a minute or even a minute. Flashlight photographs were taken while the bell was ringing, and they revealed the medium sitting in his usual hunched position, with his head sunk forward, his hands held, and his knees between the knees of the controller. Nothing was to be seen in the path of the infra-red beam.

It seems therefore that whatever it was that obstructed the beam, it was not an ordinary solid obstacle. Whatever its nature, it seemed to fade away under the influence of ordinary light: for lateral illumination of the beam, although it did not reveal anything, had the effect of promptly stopping the ringing of the bell.

Although the infra-red beam was intercepted as if something was trying to reach the handkerchief or other object, the latter was not often moved, and the experimenters wisely decided to abandon this as a primary object of study, and to concentrate attention on the phenomenon that was more easily obtained, namely the obscuration of the infra-red ray. The bell was replaced by a galvanometer with a photographic recording drum adapted to give a continuous graph of the deflections. The obscurations previously indicated by the bell were now photographed on the drum, which gave a record of intensity and duration.

After the work had been in progress for some time the ordinary galvanometer which had been in use was replaced by one of quick period ($\frac{1}{10}$ sec.) with the view of examining in more detail how the obscuring action set in. [I must explain for the benefit of readers not versed in physics that scientific measuring instruments are not effective for recording changes appreciably more rapid than their own free period of vibration. Instruments of long period are too sluggish to follow quick changes.] With the quick acting galvanometer, a very significant fact was noticed. When the ray was (partially) obscured, it was seen that the galvanometer spot moved in sympathy with the loud and rapid breathing of the entranced medium. The expiration and the inspiration each involved a muscular effort: and the number of obscurations of the infra-red ray corresponded with the number of these muscular efforts. It was clear therefore that the obscuring action was connected directly or indirectly with the medium's muscular processes. This fact alone seems to rule out confederacy.

When the above relations had been recognised arrangements

were made for automatically recording the motions of the medium's chest on the same chart as the infra-red absorptions. Several such charts are reproduced by the authors, and allow the frequency relation to be verified. For example when the infra-red absorption has a frequency of 5 per second, the breathing has a frequency of 2.5 per second.¹

These graphs published by MM. Osty are in my view one of the most valuable contributions ever made to our subject. The critics who have discussed their work adversely appear to me to have totally failed to face up to them : indeed many of the criticisms that I have seen are completely answered in advance by the original publication, so that it hardly seems worth while to answer them further. The great value of the graphs is that they carry their own tale, and that every student can examine them for himself, nearly as well as the original experimenters could do. The destructive critics have here an unrivalled field for the exercise of their talents, but so far they have not made any effective use of it.

I have read a criticism which suggests that Schneider made fraudulent use of the phenomenon of resonance. It is suggested that he timed his breathings in such a way that the movements of his body were in unison with the free period of swing of one of the supports of the apparatus. It is assumed that a large oscillation of the support was worked up in this way, enough to secure that only part of the beam entered the photo electric cell, thus simulating an absorption. I do not say that a suspicion of this kind is unreasonable *prima facie*. On the contrary, it is the sort of thing that constantly haunts the night thoughts of the careful experimentalist, and should cause him to look to the supports in the morning, and to see whether a vibration can in fact be worked up in the way suggested. Unfortunately that sort of test can only be applied at the time : but there are other tests which can be applied to the graphic records. One point is that, on the view suggested, the frequency recorded must be rigidly equal to the frequency of the free vibrations of the support, and therefore constant as long as the arrangements are unaltered. This test is not satisfied. For example,² on June 11, 1931, at 22 h. 49 m. the rate was 5 absorptions a second, and three minutes later, the rate was 8 absorptions a second.

Another point is that on this hypothesis the mechanical oscillation is gradually worked up, and therefore that its effect progressively increases. The successive absorptions ought to increase regularly in

¹ Complete periods are always referred to, *e.g.* in breathing, from the beginning of one inspiration to the beginning of the next inspiration.

² *loc. cit.*, p. 125.

amplitude and then gradually die down. But when we examine the graphs, we find nothing of the kind. In the first absorption of a series they have an average value, and exceptionally large absorptions occur quite sporadically, and not as a culmination. So far from dying down gradually, in one case the very last absorption is much the most intense.

In various criticisms it is suggested that Schneider had a confederate in the room, or that he succeeded in evading control. In some investigations, for example, those concerned with Eusapia Paladino, such questions were fundamental. I do not think they are so here. It is useless to suggest that Schneider might have evaded control unless that will help us to explain how he could have produced the periodic absorption at this very high frequency, in exact time with his breathings. Again, as regards the hypothesis of confederacy, I have already pointed out that the connexion with his breathings indicate that *he*, and not a confederate, is producing the effects.

The point that specially appeals to me is that we have a definite correlation between two phenomena, the deep breaths taken by the medium, and the absorptions of the infra-red ray, which correlation depends on a very large number of repetitions of each phenomenon. So much of what has been done in this field, however earnest the attempts of the experimenters, has only led to casual and sporadic observations; not helping, so to speak, to connect any two pieces of the puzzle. Here, however, two pieces of it are definitely connected: the loud rapid breathing and the periodic infra-red absorptions.

If it is objected that this connexion is a trick carefully arranged by Schneider, we must remember that the connexion itself was not perceived until well on in the investigation a quick period reflecting galvanometer of the necessary high sensitivity was procured. This is an out-of-the-way instrument (I doubt whether most University professors of physics were aware that such an instrument was on the market, at any rate I was not aware of it), and even when it was procured the above connexion was only noticed accidentally. To suppose that Schneider knew about quick period reflecting galvanometers, and laid his plans in anticipation of this sequence of events, would on the whole be absurd.

What, it will be asked, is the nature of the substance or agency that obscured the infra-red ray in these experiments? Is it the same as the material of the "psychic structures" in Crawford's experiments? Or is it identical with the "ectoplasm" of which we have heard so much? I am afraid I must decline to enter upon

these questions, which are altogether premature. I am not sure that Crawford's conclusions are correct, and I do not know of any observations on ectoplasm under really satisfactory conditions. We can and probably shall speculate about it in our own minds: but the Society's publications should not be encumbered with guess work. What is wanted is an independent repetition of Osty's experiments.

In the meantime, it is necessary to emphasise what seems to be a serious difficulty, when we come to consider the properties which Dr Osty finds in the invisible substance.

So far as we may claim to know anything about the action of light, I think we may say that it cannot modify matter without being absorbed in the process. Many illustrations of this principle might be given, but I will limit myself to one. Ordinary photographic plates are sensitive to blue light: and corresponding to this we find that the sensitising substance, bromide of silver, absorbs blue light. On the other hand, yellow and red light are not absorbed, and we find that the plate is not sensitive to these kinds of light. We may make it sensitive to them by staining it with certain dyes. But in doing so we have introduced an absorbing power for red and yellow light, so that the principle still holds.

Dr Osty's whole investigation depends on the fact that infra-red rays are absorbed by the "invisible substance". He considers that under the condition that prevailed this absorption was effective for waves of length 1μ ($\mu = \frac{1}{1000}$ of a millimetre) upwards, and he emphasises that shorter waves were not absorbed,¹ though he thinks that under other and better conditions they might be.

Further, the fact that the substance is "invisible" shows that it does not absorb ordinary visual light. If it did so, as, *e.g.* iodine vapour does, it could not of course be invisible, but would be seen dark on a luminous background. Or if it absorbed by lateral scattering, like an ordinary aqueous mist, it would itself appear as a bright cloud when illuminated.

But if the substance does not absorb visible light, how can it be that visible light destroys it, or at all events makes it ineffective? ² If we accept the principle which I have explained, that light can only modify a substance when that substance absorbs it, then an invisible substance cannot be destroyed by visual light. Until this paradox is cleared up, it cannot be said that we are standing on firm ground.

¹ Myers Memorial Lecture, 1933. *Supernormal Aspects of Energy and Matter*, p. 21.

² See above, p. 13.

The supernormal knowledge which, as all real students of the subject are now pretty well agreed, is shown by some mediums in trance, is largely admixed with incorrect or nonsensical matter, or with the attempt to get information from the sitter by "fishing" and attempting to pass it off as supernormally acquired. It is an empirical fact that the attempt (whether genuine or fraudulent) to produce apparently supernormal physical happenings frequently accompanies these trance utterances, and the trance personality without doubt wishes to claim the credit of having produced them. If we accept the view that these are occasionally genuine, then it would be natural to regard the fraudulent tricks played in trance as having the same relation to the genuine phenomena as the worthless utterances have to those which show supernormal knowledge. The trance personality, we may suppose, is willing to deliver genuine goods, whether of the mental or physical kind : but if not successful in doing so, it will produce the fraudulent ones rather than none at all.

This line of thought, it seems to me, should make us less ready than we otherwise might be to at once discard all phenomena produced by a medium who has once been detected in fraud. If we had at once discarded the whole series of trance utterances or automatic writings in which one misstatement of fact had been detected, the Society's labours would have been less fruitful than we venture to hope they actually have been.

I am inclined to sum up by saying that while a great part of what has been reported under this head, *e.g.* slate writing, materialisation of complete figures, is fraud, deliberately prepared by impudent imposters, and having nothing to do with the phenomena of trance, yet there is another part intimately bound up with the trance personality. Much of this latter is likewise fraudulent, but there seems to be an appreciable residue which has not been successfully dissolved by the acid of destructive criticism which has been so freely poured over it. The evidence seems to stand, and if we dogmatically reject it we shall be open to the reproach of laying down what *ought* to be the order of nature, instead of observing what *is*. If it is difficult to reconcile with our other notions, that may only be because these require to be revised or extended. Physical science has had to make adjustments of that kind often enough in the last few decades, and it would be rash to conclude that we have reached finality. But if adjustment is really necessary it will hardly be made until we have the phenomena under control for detailed examination to a much greater extent than has usually been attained. The investigators have usually been driven to give in to

every whim of the trance personality, to submit to producing noise, and to wait for interminable hours in the darkness. They have been threatened with dire results if they ventured to touch, or examine without permission. It is almost impossible to find out anything definite under these conditions : and I cannot but agree with Dr W. F. Prince and others who have insisted that it is a waste of time and energy to attempt it any more. That better conditions are not impossible of attainment, the work of Crawford and of Osty seems to show : and if the present unsatisfactory condition of affairs is to be bettered, it can only be by following the example which they have set. It would be of the utmost value to obtain independent confirmation of their results.

In this address I have tried to be strictly fair. It will perhaps be felt that the result is an unsatisfactory halting between two opinions. I must admit that in the course of study my estimate of the evidence has in several instances tended to become less favourable as I proceeded. Nevertheless, in some cases I am still quite unable to form any probable guess as to how the investigators could have been deceived.